

14 December 1960

L-25032

PERSONAL

Mr. W. Thornton Read
Bell Telephone Laboratories
Murray Hill, New Jersey

Dear Mr. Read:

Daniel Ellsberg has asked that I return to you your manuscript entitled "Operational Concepts," which Dr. T. C. Schelling passed on to him to read several weeks ago. (Mr. Ellsberg is in Washington at present, but expects to return to Santa Monica early next week.)

Very truly yours,

Susan T. Simek (Mrs.)
Secretary to
Daniel Ellsberg

ss
Enclosure (As noted)

12 December 1960

L-24755

Mr. Peter V. Ritner
The Macmillan Company
60 Fifth Avenue
New York 11, New York

Dear Mr. Ritner:

At Dan Ellsberg's request, I am writing to let you know that he received your most recent letter shortly before he left for a week in Washington.

He wanted me to emphasize that he appreciates your offer and will reply as soon as he returns to his office next Monday (December 19).

Very truly yours,

Susan T. Simek (Mrs.)
Secretary to
Daniel Ellsberg

12 December 1960

L-24800

Professor Thomas C. Schelling
Center for International Affairs
Harvard University
6 Divinity Avenue
Cambridge 38, Mass.

Dear Dr. Schelling:

Before leaving for a week in Washington, Dan Ellsberg asked that I forward immediately the eight copies of his Lowell Institute Lecture, The Theory and Practice of Blackmail, which you requested some time ago.

Our apologies for the delay, which was due to a mis-understanding on my part, I'm afraid.

Not Dan's.

Sincerely,

Susan T. Bizek (Mrs.)
Secretary to
Daniel Ellsberg

ss

Enclosures (As noted)

bcc: Evans/Haydon
ref. inc. #38311

UNIVERSITY OF CALIFORNIA

DEPARTMENT OF ECONOMICS
BERKELEY 4, CALIFORNIA

November 16, 1960

PERSONAL

Mr. Daniel Ellsberg
The RAND Corporation
1700 Main Street
Santa Monica, California

Dear Mr. Ellsberg:

I note that you are giving a paper, "Risk, Ambiguity, and the Savage Axioms," at the December meetings of the Econometric Society. I will be unable to attend the meetings but will be grateful for a copy of your paper.

Sincerely yours,

Roy
Roy Radner
Associate Professor of Economics
and Statistics

RR:pm

Best regards!
I hope to be at
RAND early in Dec.
R.

Send both
10-23-61
p-2173
* crude analysis report

bcc: C. N. Evans
B. W. Hayden
J. C. Hogan
ref. inc. #31667

16 November 1960

L-22977

Mr. Jason Epstein
Editor
Random House Inc.
457 Madison Avenue
New York 22, New York

Dear Mr. Epstein:

I am sorry for not answering your letter sooner; I have been out of town a great deal in the last two months. The fact is that my work at RAND has kept me recently from doing the work of revision and reorganization on my Lowell Lectures which I would like to complete before submitting them formally to a publisher.

I now hope to get down to this in January and February. Meanwhile, in response to your interest, I am enclosing a copy of the first lecture substantially as it was given; it too requires a good deal of revision. Assuming that the final book will conform closely to the lectures, it is not scheduled to be a RAND publication. Although I have not been actively submitting it, a number of publishers have expressed considerable interest in the work. However, I have postponed making any commitment whatever until I have a more finished product.

Thank you for your interest.

Sincerely yours,

Daniel Ellsberg
Economics Division

DE/ss
Enclosure (As noted)

16 November 1960

L-22978

Dr. Klaus Knorr
Center of International Studies
Woodrow Wilson Hall
Princeton, New Jersey

Dear Klaus:

Thank you very much for taking the trouble to check the references in Snyder's work. Apparently he inserted these after receiving my letter, since they correspond to the statement which he proposed to send to the Journal of Conflict Resolution. As such, they are minimal, but no doubt adequate. As you inferred, I would prefer to let them stand rather than to raise the issue of my conversation with you.

The new Journal hasn't come out, but I trust that the editor will present Snyder's letter. I regretted having to burden you with such unpleasant information, but I had come to feel that the policy of maintaining it a secret between Snyder and me had now become inappropriate from my point of view.

There may be a happy consequence to the affair, if it prods me to drop other work and get my lectures published: which is possible. A number of publishers have made offers for it, which has put me in the absurd position of acting coy, simply because I hadn't wanted to think about marketing it till I had a more finished product. At this point I am well disposed toward McGraw-Hill, since I am more familiar with their line, but I would appreciate any advice on the criteria for choosing a publisher, since I know very little about it. (Chicago, Prentice-Hall and Harvard have also made offers; Macmillan, Little Brown and Random House have shown interest, without having yet seen any manuscript.)

For your interest, I am enclosing a copy of the first lecture, which is the one with greatest overlap with Snyder's paper (not the only one). It is substantially as I gave it (you may recall seeing it in manuscript form) and needs a good deal of revision and reorganization. In fact, I haven't wanted to submit anything formally to the publishers till I had a chance to rework the lectures as a whole. Incidentally, this particular lecture, being more expository than the rest, rather stands by itself, and if you would be interested in a shortened and revised version for World Politics, I'm sure no publisher would object.

Dr. Klaus Knorr

-2-

16 November 1960
L-22978

Thank you again for reassuring me on the references. I enjoyed the evening at your home very much. I hope you will be paying R&M a visit before too long, so that I can repay some of your hospitality.

Yours sincerely,

Daniel Kilsberg
Economics Division

DK/ss
Enclosure (As noted)

ref. inc. #34186

16 November 1960

L-22979

Professor Marc Merlove, Associate Editor
JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION
Department of Economics
Stanford University
Stanford, California

Dear Marc:

Please forgive my taking so long to answer your request. (Frankly, I've been beginning letters all morning in this fashion. I've been away from RAND most of the time for the last two months, and today is the day for apologizing to all my correspondents.)

What is less forgivable is that after this ~~day~~, I'm afraid that I must decline to review Richardson. In fact, if I had answered your note first, I would probably have said yes, but after reviewing the stack of commitments on my desk I've become prudent. However, since I feel guilty for having delayed you, if you can't find anyone else, after trying, I will accept the responsibility to produce something -- though not by January 15th. How about Harry Bowen or Schelling?

I understand that Kennedy offered Stevenson the chance to nominate him at the convention. Stevenson asked for two days to think it over. He took four; and then said no. Apparently it would have been all right to take four days and then say yes, but saying no in this fashion was the not-done thing. If Stevenson is not Ambassador to the UN, this is why. But I share his problem.

Yours,

Daniel Ellsberg
Economics Division

DE/se

16 November 1960

L-22938

Mr. Edward Kuhn, Jr.
Editor in Chief
McGraw-Hill Book Company, Inc.
330 West 42nd Street
New York 36, New York

Dear Mr. Kuhn:

I am very embarrassed to realize how long your letter has awaited reply. I have no adequate excuse, but in extenuation, your letter arrived while I was on vacation and I have been almost continuously away from RAND throughout the fall (in Hawaii, Princeton, Boston and Washington). I am afraid that at this point I owe apologies to more than one correspondent. In any case, please let me assure you that I appreciated your taking the trouble to answer my question in such detail.

Your list of accomplishments in the trade field leaves me, naturally, speechless: which is one reason why your letter was a hard one to answer. Another is that, at this moment, I really have no manuscript in a form which I would like to submit to you. I have five or six chapters roughly in the state of the one I gave to Seymour Harris, but I would prefer strongly to revise and reorganize them before showing them. In other words, if Harris had not shown that draft to you, you probably would not have heard from me for a while.

When I first talked to Alden Paine last summer, I hoped and expected to get down to the business of revising the lectures in August. However, I became involved just at that time in several projects at RAND which have not only occupied all my time but which required me to be away a great part of the time. These should be ended by January. There is, of course, a chance that similar interruptions will appear then, but I will say that my own resistance to them is growing, along with my desire to complete this work at last.

Once I do have the opportunity to work on the book -- which should be after Christmas -- it should take me only a month or six weeks to put the lectures as I presented them into a form that I would be happy to expose to you. The question might arise then of adding material on issues I have been considering since delivering the lectures (March,

Mr. Edward Kuhn, Jr.

-2-

16 November 1960

L-22938

1959); if this seemed desirable, the new material would take somewhat longer to write.

As it happens, several other publishers have expressed interest in the book (Harvard Press, Chicago University Press, Prentice-Hall, Random House, Macmillan, Little Brown -- Chicago and Prentice-Hall having made definite offers), having either heard of the lectures or seen the first chapter in the same haphazard manner in which it came to your attention. I have avoided making any commitments, since I really wanted to postpone such considerations until I had a more finished product. I might say that I am particularly receptive to McGraw-Hill, both because I am especially familiar with your line and because I appreciate the interest you have shown.

As I said to Alden Paine last week, I think the next move must wait until I can submit to you a revised form of the lecture material already written; this would be about 160-200 pages, and, if no new commitments intervene, should be finished in February. I would hope to take advantage then of your offer of a specific critique. If Khrushchev makes good his offer to heat up the Berlin crisis in April, a book on "The Theory and Practice of Blackmail" should begin to look rather topical about then.

Sincerely yours,

Daniel Ellsberg
Economics Division

DE/ss

bcc: C. N. Evans
B. W. Haydon
J. C. Hogan
ref. inc. #25474

green

ben: C. H. Evans
B. W. Dayton
J. C. Hogan
ref. inc. #31086

16 November 1960

L-22939

Mr. Peter V. Ritner
Editor, Trade Department
The Macmillan Company
60 Fifth Avenue
New York 11, New York

Dear Mr. Ritner:

Let me apologize for taking so long to answer your letter; I have been away from RAND almost continuously for the last two months. In fact, I've been in New York several times and would have been delighted to see Andy Norman if I had known he was there.

The lecture to which Andy was referring was entitled, "The Political Uses of Madness." It was part of a series of Lowell Lectures entitled, "The Art of Coercion: A Study of Threats in Economic Conflict and War," given at the Lowell Institute in Boston in March, 1959 and broadcast later over WGBH. (Some other lectures were entitled: "The Theory and Practice of Blackmail," "The Threat of Violence," "Bargaining by Ultimatum.") I don't know how Andy heard of these; but if you found his version of my ideas fascinating, I won't disturb the impression by expounding on it.

The fact is that I have not wanted to submit the work to a publisher until I had had a chance to revise and reorganize the lecture material, a process which would take about four to six weeks once I was able to start on it. I had hoped to do this during the fall, but it now looks as though I couldn't begin until January or February. Meanwhile, the only lecture of which I have copies is the first one, "The Theory and Practice of Blackmail." I am sending it along in the form in which I delivered it, although it too needs a good deal of revision.

As it happens, several publishers have expressed interest in the book, either having heard of the lectures or having seen this chapter by one route or another (McGraw-Hill, Prentice-Hall, Harvard Press, Chicago University Press, Little Brown, Random House). I have not made any commitments of any sort, mainly because I feel ignorant of the differences between publishers and wanted to postpone this problem until I had a more

Mr. Peter V. Ritner

-2-

16 November 1960
L-22939

finished product. However, if you feel it worth your while to add to my education on the criteria for choosing a publisher, I would appreciate your efforts.

Please give my regards to Andy; I would like to know his whereabouts.

Sincerely yours,

Daniel Ellsberg
Economics Division

DE/ss
Enclosure (As noted)

Top Secret Control Office

11/7/60

Daniel Ellsberg

M-6280

DISCUSSION OF TOP SECRET MATERIAL

On Wednesday, November 2, I addressed a meeting, including the people listed below. The purpose of this session was for me to practice a presentation I was to give to the Board of Trustees.

The presentation has to do with the relationship between Command and Control systems and national objectives. One part of the presentation included some illustrative examples of practice tests of the communications system in the Pacific. These examples are Top Secret, and I have knowledge of them because of a recent job I did in the Pacific in collaboration with ONR.

All of the people at the meeting were told that the information was Top Secret, and the complete list of attendees is as follows:

Baran, Paul	Hitch, C. J.
Becker, A. S.	Hoag, M. W.
Bristol, R. B.	Hoffman, F. S.
Brock, R. L.	Ikle, F. C.
Brown, Wm.	Jackson, V. G.
Burke, T. F.	Johnson, L. L.
Carne, J. B.	Kahn, Herman
Eldridge, F. R.	Kershaw, J. A.
Ferguson, A. R.	Wolf, Chas.
Freeman, C. L.	

DE/ss

31 October 1960

L-21753

Lt. Colonel Wm. M. Jones
Air Battle Analysis Division
Office, Deputy Chief of Staff,
Plans and Programs
Headquarters, U.S. Air Force
Washington 25, D.C.

Dear Bill:

Attached is a copy of the paper I spoke to you about on my last trip to Washington. If your time permits it, I would appreciate any comments and/or criticisms you may have to offer. (I'm sure you are aware that the paper should be considered as a working paper only, and should not be quoted or abstracted without specific approval.)

Best regards.

Sincerely,

Daniel Ellsberg
Economics Division

DE/ss

Enclosure:

"Strategic Objectives and
Command Control Problems,"
August 12, 1960

bcc: AFDAP/R
Belzer/Clement
Gorham
Henderson
Mengal
Publications (D-7838)

30 September 1960

L-19373

Dr. Avery Leiserson
THE AMERICAN POLITICAL SCIENCE REVIEW
Vanderbilt University
Nashville, Tennessee

Dear Dr. Leiserson:

In answer to your request, I would be happy to review Rapaport's book, Fights, Games and Debates, for the Review. However, I expect to be away on business trips throughout much of October so I am not certain that I will be able to meet a November 30th deadline. If this possibility does not deter you, I will look forward to receiving the book.

Very sincerely yours,

Daniel Ellsberg
Economics Division

DE/ee

bcc: Evans/Haydon
ref. inc. #29677

C. A. Weihe

J. A. Kershaw

REQUISITION FOR OFFICE EQUIPMENT

Daniel Ellsberg

Will you please arrange to have two six-shelf bookcases installed in Room 2018, at the same time removing the blackboard and one of the two smaller (two-shelf) bookcases; these may be returned to stock.

Your usual prompt attention to this matter will be appreciated.

JAK/ss

green
M-5540
9/30/60
-1-

MEMORANDUM

TO: R. L. Belzer, J. B. Carne, F. R. Eldridge, Jr., C. J. Hitch,
N. C. Dalkey, G. Shubert, R. L. Van Horn

FROM: Daniel Ellsberg *DE*

SUBJECT: A SUGGESTION FOR RAND RESEARCH ON COMMAND AND CONTROL

I propose a project to design a "High Confidence (Low Vulnerability, High Reliability) Command and Control System." This would be similar to the notion of a "backbone" system, but without the usual presumption of a "minimal" system. In other words, instead of starting by defining a "minimal" set of "essential" functions (such minimal essential sets always seem to narrow down to the assured capability to transmit a "Go" signal), the goal would be to discover ways of preserving as many important capabilities as possible, consistent with the High Confidence requirement. In particular, such a study would aim at systems which would preserve with high confidence capabilities beyond the ability to transmit a single Execute order.

Examples of capabilities worthy of consideration appear on pp. 58-59 of my D-7838. Some specific capabilities that seem to deserve the High Confidence requirement might be:

1. The preservation of some high-level decision-makers (the command post problem; need not imply the preservation of one, particular CP, though this is worthy of consideration).
2. Provision to decision-makers of prompt information on post-attack status of friendly forces, at least in gross form (damage to bases, carriers, subs; numbers and locations of planes and missiles surviving and their capabilities).
3. Prompt information on nature of enemy attack; pattern of targets (e.g., are cities direct targets, subject to bonus damage, or are they being deliberately avoided); type and number of vehicles; extent of attack outside ZI; ground bursts.
4. Strict positive control of all vehicles, at least so long as main command/communications structure survives; safeguards against accident, false alarm, unauthorized action.
5. Individual, selective control of all vehicles.
6. Ability to transmit "Stop" or "Shift target" order to vehicle throughout interval during which it is physically possible to stop execution or to shift target.

7. Acknowledgment from strike units of receipt of execute orders and extent to which compliance is feasible and in process; prompt information on effects of attack upon enemy target system.

The above list may look long, yet component studies already done at RAND would suggest that these particular sample capabilities might indeed be provided with high confidence (e.g., by "bomber alarm-type" systems, by rocket and other communication systems, by preserving MIDAS and SAMOS readouts post-attack).

High confidence of surviving what level of attack? (a) At the least, a "small" attack against the C&C system: resulting from an enemy unauthorized action, an nth country action, or the initial stage of a poorly-coordinated enemy attack. (b) A major attack against US forces, with the C&C system subject to bonus damage or to a small attack. With lower priority in the research, investigate the possibility of protecting at least some functions with high confidence against (c) a major attack upon C&C capabilities.

The purpose of the High Confidence requirement is to act as an incentive to continued research effort so long as this capability is not achieved. Moreover, I am suggesting that only very loose budgetary constraints be applied in the initial research stages. Once feasibility has been established for a given, high confidence capability -- which is the initial goal of this project -- we can then look for alternative, perhaps more efficient, methods for achieving the same end: in other words, conduct cost-effectiveness studies. In fact, we might then be willing to trade some reliability for much lower costs; but such trades should come toward the end of the research, not toward the beginning. It is the purpose of the research to discover and demonstrate the feasibility of protecting certain functions to a high degree.

If, in the end, it appears infeasible to achieve high confidence for certain functions -- e.g., reconnaissance of enemy forces, communication with enemy leaders -- they need not disappear from the study. They might be listed as "lower confidence" capabilities, part of a second- or third-order system. This might be the most practical approach to generating Dalkey's "nested systems."

In the first cut at the problem, functions might well be considered individually and incrementally for preservation. The criterion for including them would not be that they stand at the head of the list of priorities (that is the usual "backbone" approach) but that they are somewhere in the list of important functions and might be preserved with high confidence. This could result in a bundle of capabilities, not clearly all serving a well-defined common purpose; but that isn't necessarily bad. To get going, it is enough to recognize that certain capabilities would serve many possible systems and objectives; research to achieve them would be worthwhile even though the precise use to be made of them could not be specified in advance. For example, if a commander is to make any post-attack decisions (and the basic assumption of this study would be that we might want him to), he will

want to know promptly and reliably the status of his forces after attack. We can set out to assure that information without figuring out in advance just what decisions he might base on it. Incidentally, a mere extension of the bomb-alarm system -- modified to provide more precise information on the nature, pattern and success of the attack -- would probably go a long way toward providing this, and could probably be achieved in the relatively short run.

If it seems desirable to aim toward a system capability right from the beginning, I suggest that an objective might be to provide the capability to support intrawar threats and bargaining. This would presumably comprise knowledge of the remaining status of one's own and the enemy's military capabilities, selective control of withheld forces, knowledge of the nature of enemy attack, perhaps ability to communicate with the enemy. Such capabilities might be achieved in a rough but significant form even in the relatively short run.

In contrast to the approach proposed here, I would characterize a typical "backbone" approach to be: "What are the minimum essential functions? How can we protect them better than at present?" With this point of view, there is a tendency to restrict consideration to a very small set of functions, and to be satisfied with "solutions" that may provide minor improvements in protection, perhaps at the cost of foreclosing many capabilities of a sort not examined. Also, particularly if the agency doing the study is interested mainly in short-run improvements and is used to severe budget constraints, "feasibility" tends to be closely related to "cheapness."

The approach I suggest is: "What post-attack C&C functions would be important under a significant set of post-attack conditions? Which of these -- how many, and how -- is it possible to preserve with high reliability (at least, under attack conditions [a] or [b] above)?" The first solutions need not, and probably will not, look cheap; they might look "economically infeasible" from the point of view of any subdivision of the DOD, or even with respect to the current DOD budget. But once having shown technological feasibility, there would be new incentive to find other, better, cheaper methods of providing this non-minimal capability; other agencies besides RAND could surely contribute to that task, perhaps with RAND playing a major part in cost-effectiveness study. But the biggest initial contribution RAND could make would be to provide a basis for "informed optimism" as to the prospects of a post-attack control capability.

RAND work on C&C so far has concentrated heavily on providing a High Confidence capability for deciding on and issuing the Execute order. My basic pitch is that we should now see what can be done to spread the High Confidence mantle over more post-attack capabilities; and more, and more. (This can still be done in a careful, step-by-step way, without starting with the assumption that the entire set of Big L capabilities "can and must" be preserved with high confidence).

After such a project has turned up a bundle of reliable post-attack C&C capabilities, we may hope that the Counterforce and the Aerospace Defense projects will have more to say as to what use might be made of them. It is

not necessary for the C&C project to try to anticipate all the results of those projects. Assuming that they will have some results, it is possible to design certain capabilities which are likely to be useful. But the C&C project should explicitly be conceived as forming part of a larger investigation of war-fighting capability; it need not draw within itself all the aspects of that investigation.

Another investigation that RAND might undertake would focus upon crisis capabilities: e.g., the ability to augment intelligence/reconnaissance/surveillance, to reduce force vulnerability, to convey threats, warnings, reassurances in a crisis. An important part of that study might be Crisis Communications. Special operating procedures, techniques of communication, augmentation of hardware and personnel, might become feasible and desirable in a crisis though not for normal operations. A related problem is the provision of reliable communications in areas outside of normal operating regions, as in limited wars and certain cold war situations.

Note that if the C&C system provides sufficient "stability" (as discussed in D-7838), an ambiguous situation in which several bombs had exploded in the ZI (ambiguous in that the nation of origin, or the enemy intent to launch all-out war, might not yet be established) might still be regarded as a period of "crisis," with all the characteristics, tensions, pressures and opportunities associated with that notion. (With a collapsible C&C system, such an occurrence would lead either to total paralysis of decision, if the bombs were in the right places, or to irrevocable holocaust.) Other outfits -- SRI, WSEG -- have been working on "crisis" problems, but their crises end when the bomb alarm rings. A RAND study might have the guts to extent the crisis past the first few bombs; the properties of crises and crisis behavior might still be relevant after an unauthorized detonation, a small nth-country action or the opening bombs of a ragged enemy attack, if the C&C system could survive a relatively small attack.

Aloha!

DE/ss

22 September 1960

To: Publications

From: E. Dews

Note of transmittal: D-7947 "Possible Strategic Objectives and Alternative Military Policies" by D. Ellsberg ✓
Unclassified

The vellum typescript dated
was received by me on
and is forwarded to you today

20 September 1960

22 September 1960

22 Sept 1960

E.

Edmund Dews

cc: Author

21 September 1960

To: Publications

From: E. Dews

Note of transmittal: D-7838, "Strategic Objectives and Command and Control Problems" by D. Ellsberg ✓

The vellum typescript dated
was received by me on
and is forwarded to you today

12 August 1960

21 September 1960

Edmund Dews

cc: Author

1 September 1960

L-17473

Mr. Glenn H. Snyder
Center of International Study
Princeton University
Woodrow Wilson Hall
Princeton, New Jersey

Dear Glenn:

I have just received your letter; thank you for giving such close attention to the matter and for replying so promptly. I am leaving within the hour for a week's vacation; therefore, I will just take the time to say that your letter to Mr. Barth and the statement for the Journal both appear quite acceptable.

Please excuse this hasty reply, I will write you at greater lengths as soon as I return.

Yours sincerely,

Daniel Ellsberg

DE:jr

*Eric J. Schelling
Hagan*

The RAND Corporation

1700 MAIN ST. • SANTA MONICA • CALIFORNIA

25 August 1960

L-16952

Dr. Glenn H. Snyder
Center of International Studies
Woodrow Wilson Hall
Princeton University
Princeton, New Jersey

Dear Dr. Snyder:

Your article, "Deterrence and Power," in the latest issue of the Journal of Conflict Resolution, has just come to my attention. As you must have expected, I am considerably dismayed by the form in which you have chosen to acknowledge my contribution to this work. So far as I can tell, its content is substantially the same as in the draft which I saw in the last week of February, 1959. It was then (and remains now) strikingly similar in content, approach, and even order of presentation to my lecture, "The Theory and Practice of Blackmail," which you had read, prior to writing your draft, during the month (February, 1959) we shared an office at the Center of International Studies. I was engaged then, you will recall, in completing my series of lectures for the Lowell Institute in Boston on "The Art of Coercion: A Study of Threats in Economic Conflict and War." This lecture, the first in the series, I had completed in manuscript before coming to Princeton in February.

After our discussion of the relationship of the two papers (the details of which I will recall for you below), you showed me a corrected version of the draft which included at least half a dozen footnotes attributing priority on significant points to me or specifically to my lecture; there was also a long footnote at the beginning acknowledging the overall influence of that lecture, noting the general similarity of the two treatments (a good deal of which represented independent work), and presenting the full citation to the Lowell Lectures.

I had doubts at that time as to whether it could be profitable for you to publish your paper in that form, since the influence attributed to me was evidently so pervasive. I concluded that I was unlikely to see in print any paper by you closely similar to this one in content. I find, however, that your recent publication corresponds almost as closely to my lecture as did your draft. What is infinitely more disturbing is to find that it now omits any explicit reference to my lecture, contains no citation either in the text or bibliography to my Lowell Lectures, and has lost all but one of your footnote acknowledgments of my ideas.

Dr. Glenn H. Snyder

-2-

25 August 1960
L-16952

Indeed, the one remaining footnote appears to give (in the opinion of several readers I have consulted) quite a misleading impression. The phrase, "I am indebted to Daniel Ellsberg for showing me..." suggests that my contribution was in the form of a comment offered by me upon your manuscript. It scarcely hints (nor is there any other hint) that I "showed" you this point -- the significance of which is left quite ambiguous in the reference -- by way of a written manuscript of my own, to which you had access before writing your paper. Since neither this footnote nor the bibliography even admit to the existence of my lectures, there is obviously no indication to the reader of your awareness of the close correspondence of our two treatments and the independence of my work, to say nothing of the other points of priority conceded by you in the revised draft you showed me earlier.

The publication of your paper in this form puts me in a very difficult position. Several publishers have expressed strong interest in publishing my Lowell Lectures as a book. I am currently negotiating with McGraw-Hill, in particular. But the appearance of a paper so closely parallel in content and approach must raise questions in the mind of a publisher even as to the independence of my work, quite apart from questions of priority (which I am not prepared to dismiss).

So far as the publisher's own mind is concerned I am, as it happens, in a strong position to reassure him. This particular lecture, written before I arrived at Princeton (and read there by a number of members of the Center staff) was delivered at the Boston Public Library one week after I left Princeton, as the enclosed poster indicates. That delivery was tape-recorded and subsequently broadcast over Station WGBH in Boston. In July, 1959, after having joined The RAND Corporation, I had this lecture (which contains much of the theoretical content of the series) reproduced by their blueprint process and circulated it widely for comments. I enclose one of these copies. It differs from the lecture delivered in Boston only by editing, the inclusion of some comments from later lectures in the series, and some newspaper quotes dating between March and July, 1959.

A copy of this lecture is on file with the Register of Copyrights, Library of Congress; when negotiations with my publisher are completed I will, of course, assign my statutory copyrights on the material to him. Several of the lectures, particularly this one and one (which you will also remember reading at Princeton) entitled "The Political Uses of Madness," have now been widely read in blueprint form or in manuscript. I draw your attention to the following citations: Thomas Schelling, The Strategy of Conflict (Cambridge, 1960), p. 13; Henry Rowen, "National Security and the American Economy in the 1960's," Study Paper No. 18, Joint Economic Committee, Congress of the United States, p. 41.

25 August 1960
L-16952

These facts will, presumably, satisfy a publisher as to the authenticity of my work. But he might well still be concerned about the reaction of the potential audience for my book (an audience rather well defined as the potential readership of the Journal of Conflict Resolution!) to the appearance of two treatments of the same material so close in tone and approach. This is not to deny your right to deal with this subject. I do not claim, by any means, that on every point of similarity I anticipated or influenced your thinking. Your previous paper on deterrence (which I read with great interest at Princeton) establishes your interest in the subject, your considerable stature as a contributor to it, and the independence of your thinking on a number of points which are mentioned in both our papers. But I cannot believe you will maintain that the relationship of these papers is adequately suggested in your published version. You, and perhaps only you, can resolve certain questions that must arise in the minds of readers we may share, and I am going to urge strongly that you cooperate in doing so. First, I would like you to recall the nature of your reaction when we discussed this matter at Princeton in March, 1959.

It is now eighteen months since that conversation. It is certainly no more pleasant for me than for you to dwell on the details of our discussion, which came after a month in which I had enjoyed the hospitality of your home on several occasions and the intellectual stimulation of our talk in the office that we shared. Nevertheless, since the outcome of that discussion seems scarcely reflected in the published version of your paper, I feel I must try to reconstruct it in your memory. I began, on that occasion: "I have read your draft. I am wondering what reference you had in mind to give me." You said: "I was going to mention your work in the preface, of course. What reference do you think would be appropriate?" I replied: "That would not be adequate. I regard this paper as essentially my own work." We then sat down, at my request, to examine the two manuscripts side by side with each other.

Without that draft of yours before me, I can no longer cite the specific passages for which you subsequently found it appropriate to give me credit. Perhaps you still have that draft: in that case, it should still show the large "X's" in the margin where I had indicated significant overlap with my own manuscript. The correspondence was even more extensive and obvious than is the case with the published version; there was frequently close matching in the wording of sentences and paragraphs, my key phrase "critical risk" was used throughout without any citation, and a number of examples and direct quotations paralleled. I recall your comment, after we had compared the first half dozen pages, that you "had not realized the full extent of my influence on this paper," and your suggestion that it was not necessary to continue. We did continue, at my insistence. Nearly every page, as I recall, had from one to three X's on it. Many of these, moreover, involved points of considerable substance or expository importance. Visually, the most striking similarity

Dr. Glenn H. Snyder

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(which remains striking in your present version) was the use of my formalization of the threat situation in terms of payoff matrices: in particular, 2×2 matrices. (As a matter of fact, those matrices had leapt out at me while you were working on that draft; that was what led me to examine it at the time, since they had been no part of your earlier work.)

The following day you asked me to inspect a corrected version of the draft in which you had inserted the footnote acknowledgments described earlier. (I had written out for you the full citation to the Lowell Lectures: which, by the way, appears in both of the citations noted earlier.) I agreed that these were adequate. I remember well your final remark: "I hope that this has not made you sorry that you came to Princeton"; it acknowledged, as gracefully as anything could, the regrettable fracture to our growing friendship. I was prepared to regard your readiness to make full citation as ample evidence of your good faith, your respect for my interests, and for standards of scholarships. I must admit frankly that I find your subsequent deletion of these references -- and in particular, the omission of any reference to the lectures which you had read -- incomprehensible in these terms.

No doubt your paper has gone through a considerable process of revision and polishing since our conversation. Since you lacked a written copy of my own paper, it would have been easy for you to forget the degree of correspondence between the two works (particularly when some of the more glaring similarities had been removed), and natural, perhaps, to come to doubt or deprecate in your mind the precise extent of the influence of my work. But I urge you to compare your final version carefully with the copy of my paper that I enclose. The correspondence remains startling. It may indicate to you that your paper has not undergone such fundamental changes in content, approach, or even presentation as you might offhand believe. It must surely bring home to you the urgency of a clarification by you of the relationship of the two papers.

I repeat that there are separate questions of independence and priority: not every point of similarity indicates my priority (but you are in a position to testify in every case, I believe, to the independence of my work from yours). Since your article has already appeared, it would not repay me to try to disentangle every point on which I might claim priority or to annotate your paper with specific references. However, if you intend to publish further in this field or to include this paper in a book, I call on you to examine my paper to refresh your memory, to consult your earlier draft if you still have it, and to rethink the necessity for specific footnote citation.

My more immediate concern is with the implications of your recent paper upon my own prospects for publication. As you will understand when you have re-read my paper, I have a vital interest in protecting the claims of my own

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work to independence (and, on some significant points, to priority). Although, inevitably, I have come to have some doubts as to the wisdom of having handled this matter privately, between us, when it first arose, I am willing (as this letter indicates) to give you the opportunity to correct the record yourself.

I ask, therefore, that you send a brief statement to the Journal of Conflict Resolution with an urgent request that it be published in their next issue. It might be in the form of a clarification, an addendum, an omitted footnote, a bibliographical reference, or what have you, but it should contain at least the following points:

- a. You wish to acknowledge having read in manuscript, before your own work was in coherent draft form, my lecture, "The Theory and Practice of Blackmail," subsequently delivered as part of the Lowell Lectures, "The Art of Coercion: A Study of Threats in Economic Conflict and War," Boston, March, 1959.
- b. This lecture, written independently of your own paper, covers much of the same material and is similar in approach and conclusions.
- c. You wish to acknowledge...[whatever degree of stimulation you do wish, on careful reflection, to acknowledge].

If you wish to explain why this addition to the bibliography did not appear earlier, I would not object to your statement that you had earlier lacked a suitable reference to this as-yet-unpublished material: or simply that it was due to oversight.

Since the publication date of the next issue must be approaching rapidly, I must ask that you give this request very prompt consideration and inform me immediately as to your intentions. On the assumption that you will agree that the above is a reasonable minimum clarification, I will not plan to bring the matter to the attention of the editors myself, pending your prompt reply, which should include a draft of the precise statement which you propose to submit to the editors. I shall inform you immediately upon receipt of this whether it appears satisfactory. You should feel free yourself, of course, to show this letter -- and the accompanying paper -- to anyone from whom you care to seek advice. To indicate the degree of my concern, let me say that I am more than willing to come to Princeton to discuss this directly with you, if that should appear called for.

Sincerely yours,

DE/ss
Enclosures (As noted)

Daniel Ellsberg
Economics Division

July 28, 1960

L-14842

Professor Myron Joseph
Carnegie Institute of Technology
Schenley Park
Pittsburgh 13, Pennsylvania

Dear Professor Joseph:

I am answering your letter of July 8 to Dr. Hitch, at his request. As you are undoubtedly aware, there has been very little work of a truly experimental nature on bargaining done anywhere, and virtually none at RAND. Professor Thomas C. Schelling, now at the Center for International Studies at Harvard, did do a large and important series of papers for RAND on theoretical aspects of bargaining behavior. Moreover, he has shown a continuing interest in quasi-experimental work, using questionnaires to demonstrate the possibility of tacit coordination, and simple game-situations to examine tactics of tacit collusion. His RAND papers (which constitute virtually the total recent RAND output on this subject), have been collected, in a more finished form, in his recent book, The Strategy of Conflict (Harvard University Press, 1960).

Lester B. Lave, a summer consultant in the Economics Division at RAND this year, has written an honors thesis for Reed College entitled, "Applications of the Theory of Games to Economics," reporting on some experiments he conducted with game models, the "prisoner's dilemma" model in particular. He may be reached at The RAND Corporation.

As for non-RAND work, you may already be familiar with the following items:

1. Jeremy J. Stone, "An Experiment in Bargaining Games," Econometrica, April, 1958.
2. Alvin Scodel, M. Sayer Minas, Philburn Ratoosh and Milton Lipetz, "Some Descriptive Aspects of Two-Person Non-zero-sum Games," Journal of Conflict Resolution, III (1959), 114-19, and IV (1960), 193-97.
3. Sidney Siegel and Lawrence E. Fouraker, Bargaining and Group Decision Making, (McGraw-Hill, 1960).

Professor Myron Joseph

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4. Donald Davidson, Patrick Suppes, Sidney Siegel, Decision-Making, An Experimental Approach (Stanford University Press, 1957).

I have myself been working in the field of bargaining and threat-behavior (and have had useful discussions with John Dunlop on these subjects), and I could easily supply references to other work which is, like mine, non-experimental. The above references, however, exhaust the experimental work with which I am acquainted.

Yours sincerely,

Daniel Ellsberg
Economics Division

DE:mm

Arnold Mengel

7/27/60

Daniel Ellsberg

M-4147

CARNEGIE TECH SEMINAR ON ARMS CONTROL

J. A. Kershaw, H. S. Rowen

On June 28, 1960, I delivered a lecture to Session III of the Preliminary Seminar on Arms Control under the Carnegie Program on Science and Government at the California Institute of Technology.

My talk was entitled "The Theory and Practice of Blackmail," and was based upon a lecture of the same title which I delivered publicly in a series of lectures for the Lowell Institute in March 1959. This was before I came to RAND as a permanent employee, and the lecture in both cases was based entirely upon unclassified material.

The subject is an analysis of patterns of threat behavior: in particular, the requirements that a threatener must fulfill for his threat to be effective.

DE/ss

27 July 1960

L-14773

Mr. Edward Kuhn, Jr.
McGraw-Hill Book Company, Inc.
330 West 42nd Street
New York 36, New York

Dear Mr. Kuhn:

I appreciate very much your interest in my manuscript, The Art of Coercion. I am, of course, familiar with the excellences of McGraw-Hill as a publisher of professional works. I admired in particular your work on John Williams' book, The Compleat Strategyst, which does have some similarities in subject matter and approach to my own work; I would be delighted if the public should receive mine as favorably.

One question in my mind is whether a publisher such as Harper's, for example, might be more suitable for a book which might seek to reach beyond a strictly professional audience. I have hoped that my work might appeal not only to political scientists and economists but, as you expressed in an earlier letter, to "intellectual laymen" as well. While I have the highest admiration for your selection and production of technical books, I would be interested to learn how you would rate your advantages in reaching this broader audience, in comparison to a publisher which concentrates primarily upon it.

Several projects at RAND have reached further into the summer than I expected and have postponed my work in completing this manuscript. However, I am much encouraged by the interest which you have shown and I do expect to get to it by the end of next month. I might say that my superiors at RAND have also encouraged me to complete this work. However, the current understanding is that, since the bulk of the work was done before I came to RAND, I will attempt to complete the editing and any additional work needed on my own time, so that the book will not be considered a RAND publication.

As the first chapter indicates, a good deal of the remaining chapters will involve a description of Hitler's techniques and successes with the use of ultimatums in the 1930's. These will be interpreted on the basis of concepts similar to those introduced

Mr. Edward Kuhn, Jr.

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27 July 1960
L-14778

in the first chapter. This material will thus serve as an application of the theoretical approach. As it happens, the material seems to have an inherent interest as well, and also presents a fairly direct analogy to some current threat situations.

Finally, I expect to discuss the role of threats in the age of thermonuclear weapons: a more extensive treatment of such subjects as deterrence and atomic blackmail. In my original lectures I used the Berlin crisis as an illustration. I would not have expected then that the same crisis would still be a timely topic now, a year later. It may be that Berlin will still be a serviceable illustration a year from now, when this book might appear.

Thank you again for your interest, and I will keep in touch with you as my work progresses.

Yours sincerely,

Daniel Ellsberg
Economics Division

DE/ss

bcc: C. H. Evans
B. V. Haydon
J. C. Hogan
ref. inc. #19640

27 July 1960

L-14777

Mr. Carroll G. Bowen
Assistant Director
The University of Chicago Press
5750 Ellis Avenue
Chicago 37, Illinois

Dear Mr. Bowen:

In answer to your question, I am making progress but not, unfortunately, on the book. I have other projects which have been occupying my time and have extended further into the summer than I expected. I hope to be working directly on the book in August, if not sooner.

I appreciate your continued interest in my work and I plan to take advantage of your offer to give additional comments as I complete more of it.

Yours sincerely,

Daniel Ellsberg
Economics Division

DE/ss

bcc: C. N. Evans (Lowell Institute Series, "The
B. W. Haydon Art of Coercion")
J. C. Hogan

ref. inc. #21875

27 July 1960

L-14779

Professor David C. Elliot
Division of the Humanities
California Institute of Technology
Pasadena, California

Dear David:

I enjoyed the exchanges at my session with your seminar group very much and I am delighted to have your excellent summary. It's not only useful for me to find out what I said but satisfying to have evidence that my main points were getting through. I compliment you on your rapporteur.

I enclose a copy of the manuscript on which my talk was based. For present purposes it should be considered a preliminary draft (I have received extensive comments on it but have not yet had an opportunity to incorporate them or reorganize it). It may be of interest to some of those who were present.

Harvy Roven and I had a very interesting and productive discussion with Matt Sands on possible biases in RAND's approach to problems, and in that of some of its critics. I look forward to an opportunity to discuss such subjects with you and other members of your seminar in the future.

Yours sincerely,

Daniel Ellsberg
Economics Division

DE/ss

Enclosure (Noted)
bcc: A. Mengel

Arnold Mengel

7-25-60

Daniel Ellsberg

M-4088

LOWELL INSTITUTE SERIES

J. A. Kershaw, J. C. Hogan

Some time during the next six months, I expect to prepare my lectures on "The Art of Coercion" (see attachment) for publication. As the enclosed poster indicates, these lectures were delivered publicly in Boston in March 1959, and subsequently broadcast over Station WGBH in Boston. Thus they were essentially completed before my arrival at RAND. Work involved in editing and completing them for publication will be done on my own time without the use of RAND secretarial help or other resources.

DE/ss
Enclosure (Noted)

29 June 1960

L-12945

Professor B. F. Haley
Managing Editor
AMERICAN ECONOMIC REVIEW
Room 220, Stanford University
Stanford, California

Dear Professor Haley:

I can understand your restriction. (I had not seen the book when I talked to you, though of course the reference did not come as a surprise.)

Unfortunately, several others that I might have suggested as reviewers (e.g., Boulding, Luce or Raiffa) are also mentioned in the preface. Among remaining candidates, Martin Shubik might be a good choice, or Carl Kaysen, Robert Dorfman, or Leo Hurwicz.

Sincerely yours,

Daniel Ellsberg
Economics Division

DE/ss

ref. inc. #20225

29 June 1960

PERSONAL

Mr. Paul Mandel
LIFE Magazine
127 East 81st Street
New York 28, New York

Dear Paul:

I can't give you any helpful comments on your game from the commercial or entertainment point of view, since I am neither a Civil War buff nor a player of board games (or any other kind of games). However, as a game-theorist, a war-gamer, and a child player of Monopoly, I had no difficulty in recognizing that your set of instructions defined a true "game."

I have been unable to find any basis for your critic's reservations on this point. His reference to von Neumann is puzzling, since "game theory" could hardly have less relevance to your problems. Perhaps he is under the impression that von Neumann and Morgenstern prescribed properties which a "proper" or "fun" or "interesting" game "ought" to have. If so, he is mistaken. They were concerned not with the design of games of strategy but with specifying appropriate behavior (which they tended to equate with extremely conservative behavior) for players. For their model of a "game," they abstracted certain essential features common to all familiar games of strategy, certainly including yours.

If this needs documentation, perhaps the most authoritative source is a recent translation of von Neumann's original 1928 article, "Zur Theorie der Gesellschaftsspiele," appearing in "Contributions to the Theory of Games, Volume IV" (Princeton University Press, 1959).

This begins:

1. The present paper is concerned with the following question:

n players S_1, S_2, \dots, S_n are playing a given game of strategy, \mathcal{G} . How must one of the participants, S_m , play in order to achieve a most advantageous result?

The problem is well known, and there is hardly a situation in daily life into which this problem does not enter. Yet, the meaning of this question is not unambiguous. For, as soon as

$n > 1$ (i.e., \mathcal{G} is a game of strategy in the proper sense), the fate of each player depends not only on his own actions but also on those of the others, and their behavior is motivated by the same selfish interests as the behavior of the first player. We feel that the situation is inherently circular.

Hence we must first endeavor to find a clear formulation of the question. What, exactly, is a game of strategy? A great many different things come under this heading, anything from roulette to chess, from baccarat to bridge. And after all, any event -- given the external conditions and the participants in the situation (provided the latter are acting of their own free will) -- may be regarded as a game of strategy if one looks at the effect it has on the participants. What element do all these things have in common?

We may assume that it is the following:

A game of strategy consists of a certain series of events each of which may have a finite number of distinct results. In some cases, the outcome depends on chance, i.e., the probabilities with which each of the possible results will occur are known, but nobody can influence them. All other events depend on the free decision of the players S_1, S_2, \dots, S_n . In other words, for each of these events it is known which player, S_m , determines its outcome and what is his state of information with respect to the results of other ("earlier") events at the time when he makes his decision. Eventually, after the outcome of all events is known, one can calculate according to a fixed rule what payments the players S_1, S_2, \dots, S_n must make to each other.

No doubt you will recognize this as an abstract description of your own game. I might mention that your game does have the properties of a special class of games to which von Neumann and Morgenstern gave special attention. First, it is a "two-person zero-sum" game (i.e., it has two players, and the sum of the payoffs to the players at the end is zero; it is impossible for both players to come out ahead or for both players to be losers, unlike War, which is a non-zero-sum game). For such games von Neumann and Morgenstern did prescribe a "rational" way of playing, which corresponds to the rule: Look at the worst possible outcome of each overall strategy and pick that strategy (which determines your play throughout the game) with the best of these minima (the "maximin" strategy). Of course, this advice couldn't be applied unless you calculated the outcomes corresponding to every one of your possible strategies paired with every one of your opponent's possible strategies: which would be impossible for your game with the fastest computer in existence.

Mr. Paul Mandel

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29 June 1960

Your readers probably wouldn't like this strategy, anyway, since it essentially takes for granted that your opponent is following the same conservative policy; it offers no hope of exploiting his "mistakes," his recklessness, or his individual style of play if he is not a "minimaxer." This is not the way people play games for fun. (I enclose an article of mine elaborating on this subject, if anyone's interested.)

Your game has the further feature that, like chess or backgammon, it is a game of "perfect information," in which each player makes each move with full information as to the outcomes of all earlier moves (unlike poker, in which the outcome of some earlier moves -- the "deal" -- is not known to all the players). Whereas the "optimal" way of playing most games (including poker) would, according to von Neumann and Morgenstern, involve "randomizing," picking one's strategy partly on the basis of a random device, von Neumann proved that games of "perfect information" are "strictly determined": there exists a non-random, definite strategy which would guarantee a win or a draw for one of the players (and therefore would be, in their terms, the "best way of playing"). However, you needn't worry that one of your competitors will publish this "sure win" strategy for your game and remove all suspense for your reader-players. This would take the currently-non-existent computational facility I mentioned above. (Thus, we know there is an unbeatable way to play chess, but even the Russians don't have it.)

I may have told you more than you care to know about game theory. Actually, some thoughts on the construction and use of war games would probably be more helpful to you, as a designer of a war game (if you look up the LIFE article on RAND, you will find me playing a board game -- I was on the Russian team -- that looks something like yours). There are some RAND papers on this subject I will try to get to you; but I won't hold up this letter for them.

Please don't apologize for prodding me; why shouldn't you want an answer, after three months? On the other hand, I won't apologize for having to be prodded. Why should you expect to get a response any faster than my boss, my creditors, or my wife could? At any rate, if your game fails to have the success that no doubt it deserves, it will not be because any game-theorists have risen to discredit it.

Yours,

Daniel Ellsberg/ss

27 June 1960

L-12702

Professor Thomas C. Schelling
Center for International Affairs
Harvard University
6 Divinity Avenue
Cambridge 38, Mass.

Dear Tom:

I would be delighted to contribute a paper to your session on Economics and National Defense in St. Louis; thank you for the invitation. I was planning to be in St. Louis, anyway, to read a paper for the Econometrics Society.

I would prefer to give something on the inter-relationship of strategic objectives; for example, conflicts and complementarities between various threat strategies (Type I and Type II, etc.). I worked on this before I got involved in command and control, and have been planning to go back to it this summer. It's a good vehicle for my various scattered notions on bargaining theory.

We applaud your good taste in choosing a name for your new son. Robert has the impression that he was named for him; I hope you don't mind. Thank you very much for the copy of your book, for the references, and for your inscription; Carol comments that you should be employed at writing honorary degree citations for Harvard. Apparently I'll be seeing you in July in Monterey; I'm looking forward to it.

Yours,

DE/ss

bcc: Eisner/Mengel
ref. inc. #19669

Daniel Ellsberg
Economics Division

8 June 1960

L-11468

Professor John R. Meyer
Department of Economics
Harvard University
Cambridge 38, Mass.

Dear John:

Thank you for your invitation to present a paper at the December meetings; I am of course delighted to accept.

The title of the paper (which is not now scheduled for publication elsewhere) will be: "Risk, Ambiguity, and the Savage Axioms." My affiliation will be: The RAND Corporation.

I have just learned that you will be out here this summer, and we are looking forward to seeing you and your wife very much. If there is anything that I can do in the way of arranging for a house, transportation, etc., please let me know.

Sincerely yours,

Daniel Ellsberg
Economics Division

DE/ss

ref. inc. #17492

Green ok

MEMORANDUM

To: Committee on Command and Control for 1970

Date: 5/25/60

From: Daniel Ellsberg

Memo: M-2915

Subject: RELATION OF STRATEGIC OBJECTIVES TO COMMAND AND CONTROL

Dist'n: F. R. Eldridge, R. A. Eldridge, H. Goldhamer, H. Kahn,
A. W. Marshall, C. B. McGuire, R. D. Specht, W. H. Ware,
J. D. Williams, C. J. Zwick

I am delighted to learn, from the memos by John and Bob, that I was not punching at empty space in my piece, since they have reiterated succinctly the viewpoint that I was attacking. Since I couldn't be mistaken, this must mean that I didn't make my argument plain enough. Let me try to clarify it, and at the same time put it in perspective.

Obviously, the task of this group is to suggest paths for RAND research on the subject of command and control, not to reach any definitive conclusions about the desirable form of the command and control system. My comments were intended to encourage a particular sort of research that I feel must be done in connection with any useful work on command and control: investigation of feasible strategic objectives. I won't argue whether this should be done as part of a "command and control project" or whether it should be part of a larger "war-fighting project" in which C&C was another sub-project. But I do think that it is mistaken to believe that much useful work can be done on designing a command and control system quite independently of any investigation of feasible wartime objectives. In other words, I distrust John's analogy of the steering wheel for a fire truck: partly because it assumes implicitly that we do know intimately the purposes of the machine we are trying to control, and partly because it implies (perhaps correctly, for this example) that the linkage is very loose between those purposes

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and the precise design of the steering mechanism. Neither, I think, holds true when we consider the wartime control of a strategic offensive force.

Before I enlarge on that, let me emphasize that the problem of objectives is by no means the only problem for research in this area (I simply happen to think that it is vital, and that it tends to be neglected in proposals for research on C&C: witness the otherwise excellent Logistics draft), nor are the other problems easy. The Winter Study Group has come to emphasize four sets of factors which have an important bearing on the design of a C&C system:

1. The number, kind, and performance of forces to be controlled; and the enemy capabilities which constitute the threat.
2. Objectives, strategies (plans for using the forces), and the nature of wartime environment.
3. Command and control technology: computer technology, communications and display means, hardenability of computers and communications, heuristic programming, etc.
4. Organization of the military forces; command structure.

At RAND, the SOFS project tended to contribute to (1). Ongoing work at RAND (e.g., under Frank) has concentrated on (3). The Logistics paper pointed out that (4) was being neglected, and proposed that RAND do both empirical analysis and theoretical work (e.g., in areas of team theory and decision theory) to investigate the advantages of different organizational modes and different decision rules for achieving system objectives.

All these are necessary, and they must be attacked pretty much simultaneously, since a state-of-the-world in one area constrains what is possible in the others. I am merely urging (what the Winter Study Group has now accepted) that the same holds true for (2).

John says: "If we can agree on the first thing about what we aspire

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to -- e.g., a fire engine, as opposed to a facade of a fire engine, or a booby trap or a doomsday machine disguised as a fire engine -- then the second thing comes easy: research on things that look pertinent and useful to the creation and ownership of fire engines." But I think that he meant that first "If" clause to be merely rhetorical. It isn't; this is the whole point of the argument. There has not been a national decision to buy a fire engine. On the contrary, what we have and what we are effectively planning to maintain is a doomsday machine. And a doomsday machine does not need a steering wheel. (Think of it as a fire engine at the brink of a steep and narrow road, whose only function is not to fight fires but to plunge down the hill when pushed.)

Bob suggests, the question of objectives is irrelevant because you need a survivable C&C system "that lives for quite a spell, say weeks" even if your only desire is to give the Go order reliably. He claims, "the choice is between a system that lives through a war and one which has an unacceptably high probability of dying before it can give the word to go." If this were true, it would indeed indicate that the problem of system design could be isolated from the question of objectives, since all objectives would require the same basic sort of system. But it is not true. Bob is clearly assuming that the Go order must be given from a "center"; hence, that it must be invulnerable if it is even to give the Go order (hence, that it could survive longer and give other orders). But if all that you want is a system that (a) Goes when the Russians attack, and (b) doesn't Go unless the Russians attack, you do not necessarily have to have a center, or any survivable war-fighting capability. All you need is a reliable bomb-alarm system, to be verified (e.g., by interrogating

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higher headquarters with rockets and consulting other indicators) and then acted upon, if and only if communications with higher headquarters disappears. These limited objectives are far from hypothetical; they are the current effective national objectives, and there is no great rush to change them.

Similarly, John maintains that the following "almost axiomatic" requirements imply a survivable war-fighting capability:

1. Most of the troops can be told when, and when not, to fight.
2. Most of them can be told whom to fight.
3. Most of them can be told how to fight.
4. The preceding requirements are to be met with probability approaching 1.

His conclusion simply does not follow from these requirements; he is making an implicit assumption about the variety of choices the system must be capable of making. Suppose that the content of these messages were specified in advance; i.e., that we had in effect only one war plan. For example:

1. When to fight: when someone launches a large attack (as revealed, say, by bomb-alarm system).
2. Whom to fight: Sino-Soviet Bloc.
3. How to fight: launch all available forces immediately against Bloc cities, and known military targets.
4. Pre-plan completely, design reliable bomb-alarm system (with acceptably low false alarm rate) and redundant, diverse communications system.

These instructions obviously do not call for a "steering wheel" (which is useful if there is more than one direction you might want to go).

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The "whom to fight" requirement might seem to pose a problem by 1970. But it might be possible to set the system response so that it would go only if there were a large attack on the United States; such an attack might not, by 1970, be of ambiguous origin. Even if it were, there are those who would argue that it does not pose an inexorable requirement for a centralized system. A highly placed colonel in the Pentagon, when I raised the question as to whom we would decide to hit, replied: "Sure, there's a possibility that a third country might have started it, but I don't think it's likely. Anyway, that's a risk you have to take. All you can do, when you're attacked, is to decide who your most significant enemies are and hit them."

This brings up a major point. Clearly this colonel would agree that this does not represent the best of all conceivable worlds; it would obviously be nice to know who had attacked us, and to tailor our responses to that information. But he is raising the issue of feasibility: as are most of the members of the Volunteer Doomsday Department. Most of them are, in fact, fire engine buffs, and they hate the thought of using their shiny, complex machine as a projectile. But they don't believe it can be made to serve any other purpose.

To provide an effective demonstration that other objectives are feasible is not really a matter (as John and Bob suspect) of becoming involved in "Madison Avenue selling techniques." It is more like setting out to prove a theorem which, on intuitive grounds, we already believe to be true. Other people have different intuitions, which they defend just as strongly. We may think that their lousy intuition is a sign of stupidity, but on the other hand we can't claim that they have ignored or rejected or

To: Committee on Command and Control for 1970

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M-2915

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failed to comprehend the rich evidence and reasoned argument we have laid before them. RAND hasn't provided any. We have just matched our intuition against theirs, and it is time to face the sad fact that we haven't swept the field with this tactic. RAND should not scorn to do the homework to defend its insights.

I will agree that once you (RAND, the Air Force, the DOD) accept the notion that it is feasible and desirable to have a war-fighting control capability which lasts past the first hours and which can choose among different strategies, then the design of this control system can proceed to some extent independently of the question of the precise objectives and strategies. But it can't proceed very far. Are communications to be burst-type (e.g., in rockets); broadcast or point-to-point; what information should headquarters receive; what information should be widely disseminated; how long must certain types of headquarters survive; what are the appropriate criteria for survivability -- at what level of attack would we be willing to let the control capability degrade to a cruder form? These and many other questions call for a fairly close liaison between C&C research and research on objectives/strategies, even when it has been decided to investigate a war-fighting capability.

Perhaps the moral of my remarks is that there is a lot to be said for subsuming RAND work on command and control under a broader investigation of war-fighting capability in general. It is the feasibility and desirability of the latter that is really in question; it includes many costs (e.g., passive and active defense) besides the costs of the survivable C&C subsystem, though the C&C costs will be a very significant fraction of the marginal costs of a war-fighting capability. To demonstrate that (or whether) these

To: Committee on Command and Control for 1970

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marginal costs are worthwhile is a project highly worthy of RAND. It is quite appropriate for this committee to support it, if my arguments are accepted as to the strong and essential interaction between C&C requirements and the other aspects of a war-fighting posture, including forces and threat, organization and management systems, passive defense, and objectives and strategies.

DE/ss

~~TOP SECRET~~

bcc: Publications
TS Control
A. Mengel

4 May 1960

L-8982

Office of the OMR Representative
CINCPAC
Box 32-A, FPO San Francisco
c/o APO #953
San Francisco, California

Attention: Mr. John Wilkes

Dear John:

This is the paper I promised to send you. There will be another on the subject of atomic control; I trust the next one won't take as long.

Sincerely yours,

Daniel Ellsberg
Economics Division

DE/ee

Enclosure

Manuscript, "The PAXON Response
to a JCS 'Execute' Message (U),"
dtd. 4/25/60, Top Secret

D(L)-7234
TS-2301

TRANSMITTAL APPROVED:

James L. Olsen
Administrative Contracting Officer
(Ref. Ind. Sec. Man. para. 121)

IF ENCLOSURES ARE WITHDRAWN OR NOT ATTACHED THE
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15 April 1960

L-7618

Miss Agnes L. Frey
Chief, Acquisition Section
Library
United States Army War College
Carlisle Barracks, Pennsylvania

Dear Miss Frey:

I appreciate the interest in my lectures on "The Art of Coercion," but I regret to say that the lectures were delivered from manuscript and have not yet been revised to a form which I am prepared to distribute. However, it does happen that I have a copy of my initial lecture, "The Theory and Practice of Blackmail." I am enclosing this, but it must be regarded as a preliminary draft. I hope in the near future to have the time to polish and reorganize this material for publication.

Incidentally, all six lectures were typed for broadcast over Station WGBH in Boston (an educational station) and if there is any interest it would be possible to borrow them from that station.

As soon as I have copies of the lectures in a more finished form I will be happy to send a set to you.

Sincerely yours,

Daniel Ellsberg

DE:rlh
Enclosure

Ref. Inv. No. 14977

15 April 1960

L-7617

Miss Mildred G. Black
c/o OHR Representative - CINCPAC
Box 324, c/o FPO
San Francisco, California

Dear Mildred:

Following is my itinerary for my trip to WESTPAC:

January 18-20	Travel
January 20-24	Japan (Tachikawa, Yokosuka)
January 25-26	Okinawa (Madura, USS St. Paul)
January 27	Taiwan (Taipei)
January 28, 29	Okinawa
January 30-	
February 9	Japan
February 10, 11	Korea (Seoul, Kunson, Osan)
February 12	Tokyo
February 12, 13	Travel

If questions arise on details of arrivals and departures or as to persons visited, refer to the itineraries of Dave Brown or Joe Replogle, which were the same except for the visit to Korea.

By the way, please reassure Georges that he did not erase any of my tapes since I did not use them after all. Please remember me to everyone in the project.

Sincerely yours,

Dan Ellsberg

DE:rlh

Ref. Inc. No. 11178

~~SECRET~~

11 April 1960

L-7206

ONR Representative
CINCPAC
Box 32A, FPO, APO #953
San Francisco, Calif.

Attention: Mr. David R. Brown

Dear Mr. Brown:

Following up the request you made last Friday, we are forwarding herewith your sheet of notes (ten pages, classified as Secret) taken during your recent visit to RAND.

Very truly yours,

Suzan T. Kinsch
Secretary to
Daniel Ellsberg
Economics Division

SS
Enclosures (Noted)

TRANSMITTAL APPROVED:

James L. Olson
Administrative Contracting Officer

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~~SECRET~~

26 February 1960

L-4002

Miss Julie Grace
Circulation Credit Department
AVIATION WEEK
338 West 42nd Street
New York 36, New York

Dear Miss Grace:

Thank you for calling to my attention that I sent you one check too many for my present subscription to Aviation Week. In fact, if it is true that my subscription would now run to November, 1965, I wonder if I have not sent several checks more than I intended.

Since it is against my nature to make plans or commitments that far in advance, I would appreciate your cutting my subscription down to two years (November, 1961, or thereabouts, depending on when I sent in my first order) and refunding me the difference.

I am sorry to have caused you this trouble.

Sincerely,

Daniel Ellsberg
Economics Division

DE/ss

26 February 1960

L-4001

Mr. Carroll G. Bowen
Assistant Director
The University of Chicago Press
5750 Ellis Avenue
Chicago 37, Illinois

Dear Mr. Bowen:

I will be happy to see you at your convenience when you arrive in Santa Monica; I suggest that you call me at RAND (EXbrook 3-0411, Ext. 218) and we can arrange a time.

Please remember me to Jack Hirshleifer.

Sincerely,

Daniel Ellsberg
Economics Division

DE/ss

ref. inc. #5881

26 February 1960

L-3999

Professor Irving Schweiger, Editor
THE JOURNAL OF BUSINESS
The School of Business
The University of Chicago
Chicago 37, Illinois

Dear Professor Schweiger:

Please excuse my delay in answering your letter; I received it only this morning on my return from a trip which kept me out of the country for over a month. Normally, I would be happy to review a book like Shubik's, which is in my field of major interest. However, in my absence, a great deal of work has stacked up on my desk, and I am afraid that commitments stretching into the distance will prevent me from fulfilling your request. A hasty scanning of Martin's book indicates that it is complex and highly original, and deserves a much closer study than I will be able to give it in the near future.

I hope that I will be able to be more helpful on some future occasion.

Sincerely,

Daniel Ellsberg
Economics Division

DE/ss

26 February 1960

L-4000

Mr. Martin Shubik
General Electric Company
445 Park Avenue
New York 22, New York

Dear Martin:

I am delighted to hear that you will be here in March, and even more delighted that you will be here all summer. I am pondering whether I owe it to my employers to inform them that you will probably regard RAND merely as a base from which to manage your properties in Baja California. I just returned from six weeks in the West, or East (Japan, Korea, Okinawa, Formosa, and Hawaii), but I may be ready to accompany you South by June.

Yours,

Daniel Ellsberg
Economics Division

DE/ss

R. T. Nichols

1/8/60

Daniel Ellsberg

PROGRESS REPORT

PROJECT 4201: STRATEGIC OBJECTIVES

On my arrival at the end of June, I was working on the SOFS project on strategic objectives. I took time off to answer a request from General Wilson for a critique of a study done in Hq USAF on ICBM force requirements. This particular critique appeared as D-6628. An 11-page commentary by Harry Rowen, which actually covered most of the points mentioned in this paper, provoked some discussion in Washington with the author of the study rejecting virtually all the criticisms on the grounds that they were not supported by evidence or references, and were too "general" to "be of any use to him." The comments comprising D-6628 were based entirely upon his written report. I had the benefit of reading proposed drafts by Tom Edwards and by Fred Hoffman, from which I borrowed several sentences; however, the "D" mainly amplified and extended the sort of points included in Harry Rowen's draft, making closer reference to the written study and to RAND literature.

Since then I've been working mainly on an analysis of strategic objectives for the Air Force; in particular I've been working on a critical analysis of Type II Deterrence and the relation of the first-strike threat to retaliatory threats. Several "D"s are in progress that will report the results in this area on such subjects as the feasibility, limitations and risks of Type-II Deterrence, and the implications of other force "insurance" for the stability of deterrence. Also in connection with this work I prepared D-6962, which deals with some possible Russian incentives for arms control proposals and the impli-

cations such proposals might have for the strategic posture of the United States. The main argument of this paper is that Russian opportunities for aggression abroad would be improved if the threat of general war is reduced; that one possible way for the Russians to promote this is by an arms control agreement; and further, that even a unilateral limitation of arms by the Russians could work in this direction.

D-6628

Daniel Ellsberg

COMMENTS ON HITCHCOCK'S "ICBM FORCE REQUIREMENTS" (U), Secret

D-6962

Daniel Ellsberg

ARMS CONTROL: STRATEGY FOR THE EAST? - Unclassified

PROJECT 4179, COMMUNICATIONS, COMMAND, AND CONTROL

During the past six months, I have done some work on the problem of the authentication of the go-order to the Strategic Air Command, and gave some assistance to Frank Eldridge on his SOFS briefing. At the present time, I am contributing to an ONR project on PACOM information and control at Camp Smith, Hawaii. I am examining strategic objectives in the Pacific area and their relation to problems of command and control in that area. I am scheduled to give a briefing on PACOM Command and Control to the Winter Study Group at RAND in February.

DE/ss

Publications

1-6-60

S. T. Simek (Econ.)

WITHDRAWAL OF D-6962

Daniel Ellsberg has asked that no further distribution be made of his recent D-6962, "Arms Control: Strategy for the East?". He plans to make some revisions upon his return from a month's tour-of-duty in the Pacific.

Thanks.

RAND-S.M.-REC'D

ELLSBERG

1959 DEC-9 PM 4:40 **CONFIDENTIAL**

1 Dec 1959

AFDAP

Study of the PACOM Information and Control System;
request for consultant assistance

ANS. BY
LETTER NO.
DATE

Chief of Naval Research
Department of the Navy

1. Reference is made to your letter dated 9 November 1959, subject as above.
2. The Air Force is pleased to comply with your request for RAND consultant assistance in the OMR study of the PACOM Information and Control System. We understand that the extent of The RAND Corporation's participation is on the order of ten man-weeks between now and September 1960. The Air Force will absorb the cost of these consultants through the existing Project RAND contract.
3. It is our understanding that the RAND participation will be in the capacity of technical consultation only. We suggest, therefore, that you obtain the necessary Air Force military representation through PACAF.
4. We are notifying The RAND Corporation of our approval of your request.

/s/

VICTOR R. HAUGEN
Maj General, USAF
Director of Development Planning
DCS/Development

Copy to Dr George Parham, AFM Wash

COPIES: R. L. Belzer
 J. R. Goldstein
 L. J. Henderson, Jr.
 C. J. Hitch
 A. S. Mengel

BP
12/7/59

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CONFIDENTIAL

Postal Registry No. 44346

Copies to persons
as indicated

19 October 1959

L-19629

The Office of Senator Hubert H. Humphrey
1311 New Senate Office Building
Washington 25, D.C.

Dear Sirs:

Thank you for sending me your memorandum on foreign policy studies and reports, and for continuing to send me reports of your Committee Hearings. (I presume that it was my friend Dan Jacobs who suggested that these reports be sent to me.)

As a member of the Economic Analysis Department of The RAND Corporation working on Weapons Systems Evaluation, I would appreciate receiving any material that becomes available in the future on the subjects of foreign policy, military affairs, and disarmament. The following items listed in your memorandum will be of great interest to me:

1. The Operational Aspects of U.S. Foreign Policy.
2. Possible Developments in Military Technology, their Influence on Strategic Doctrine, and the Impact of Such Developments on U.S. Foreign Policy.
3. Formulation and Administration of U.S. Foreign Policy.
4. U.S. Foreign Policy in Western Europe.
5. The Nature of Foreign Policy and the Role of the United States in the World.
6. The Principal Ideological Conflicts, Variations Thereon, their Manifestations, and their Present and Potential Impact on the Foreign Policy of the United States.
7. Foreign Policy Implications for the United States of Economic and Social Conditions in Lesser Developed and Uncommitted Countries.
8. U.S. Foreign Policy in the USSR and Eastern Europe.
9. SS #4 - Technical Problems.
10. SS #5 - Disarmament and Security in Europe.
11. SS #6 - Disarmament and Security in the Middle East.
12. SS #7 - Disarmament and Security in Latin America.
13. SS #9 - Disarmament and Security in Eastern and Southern Asia.
14. SS #10 - Detection of and Inspection for Underground Nuclear Explosions, Replies from Seismologists to Subcommittee Questionnaire.

The Office of
Senator Hubert H. Humphrey

-2-

19 October 1959
L-19629

15. Final Report: A Compilation of Staff Studies and Reports.
16. Controlling the Further Development of Nuclear Weapons: A Collection of Excerpts and a Bibliography.
17. Reports - 1957 Report of the Subcommittee.
1958 Final Report of the Subcommittee.
18. Hearings - Control and Reduction of Armaments (Parts 1-17).
Disarmament and Foreign Policy (Parts 1 and 2).
Geneva Test Ban Negotiations (Wadsworth Testimony).
Geneva Test Ban Negotiations (McCone Testimony).
19. Worldwide and Domestic Economic Problems and their Impact on the Foreign Policy of the United States.
20. Summary of Views of Retired Foreign Service Officers.
21. Possible Scientific Developments and their Potential Impact on Foreign Policy of the United States.
22. The Role of Multilateral Organizations in the Formulation and Conduct of U.S. Foreign Policy.
23. U.S. Foreign Policy in the Near East.
24. U.S. Foreign Policy in South Asia.
25. U.S. Foreign Policy in Africa.
26. U.S. Foreign Policy in the Far East and Southeast Asia.
27. International Medical Research.
28. Status of World Health.
29. Cancer - A Worldwide Menace.

My present address is: The RAND Corporation, 1700 Main Street,
Santa Monica, California.

Very truly yours,

Daniel Ellsberg
Economics Division

DE/ss

30 September 1959

L-18464

WORLD POLITICS
Woodrow Wilson Hall
Princeton, New Jersey

Gentlemen:

I am enclosing my check for \$10.00 to cover a two-year subscription to World Politics, commencing with the January 1959 issue.

Kindly note, also, that I wish to have the Journal sent to me here at The RAND Corporation, 1700 Main Street, Santa Monica, California.

Very truly yours,

Daniel Ellsberg
Economics Division

DE/ss
Enclosure